

Carneiro, Pedro; Locatelli, Andrea; Ghebremeskel, Tewolde; Keating, Joseph

**Working Paper**

## Do public health interventions crowd out private health investments? Malaria control policies in Eritrea

IZA Discussion Papers, No. 6560

**Provided in Cooperation with:**

IZA – Institute of Labor Economics

*Suggested Citation:* Carneiro, Pedro; Locatelli, Andrea; Ghebremeskel, Tewolde; Keating, Joseph (2012) : Do public health interventions crowd out private health investments? Malaria control policies in Eritrea, IZA Discussion Papers, No. 6560, Institute for the Study of Labor (IZA), Bonn, <https://nbn-resolving.de/urn:nbn:de:101:1-201301212776>

This Version is available at:

<https://hdl.handle.net/10419/58648>

**Standard-Nutzungsbedingungen:**

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

**Terms of use:**

*Documents in EconStor may be saved and copied for your personal and scholarly purposes.*

*You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.*

*If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.*

IZA DP No. 6560

**Do Public Health Interventions Crowd Out Private  
Health Investments? Malaria Control Policies in Eritrea**

Pedro Carneiro  
Andrea Locatelli  
Tewolde Ghebremeskel  
Joseph Keating

May 2012

# **Do Public Health Interventions Crowd Out Private Health Investments? Malaria Control Policies in Eritrea**

**Pedro Carneiro**

*University College London,  
IFS, cemmap, Georgetown University and IZA*

**Andrea Locatelli**

*University College London*

**Tewolde Ghebremeskel**

*National Malaria Control Program, Eritrea*

**Joseph Keating**

*Tulane University*

Discussion Paper No. 6560

May 2012

IZA

P.O. Box 7240  
53072 Bonn  
Germany

Phone: +49-228-3894-0

Fax: +49-228-3894-180

E-mail: [iza@iza.org](mailto:iza@iza.org)

Any opinions expressed here are those of the author(s) and not those of IZA. Research published in this series may include views on policy, but the institute itself takes no institutional policy positions.

The Institute for the Study of Labor (IZA) in Bonn is a local and virtual international research center and a place of communication between science, politics and business. IZA is an independent nonprofit organization supported by Deutsche Post Foundation. The center is associated with the University of Bonn and offers a stimulating research environment through its international network, workshops and conferences, data service, project support, research visits and doctoral program. IZA engages in (i) original and internationally competitive research in all fields of labor economics, (ii) development of policy concepts, and (iii) dissemination of research results and concepts to the interested public.

IZA Discussion Papers often represent preliminary work and are circulated to encourage discussion. Citation of such a paper should account for its provisional character. A revised version may be available directly from the author.

## ABSTRACT

### **Do Public Health Interventions Crowd Out Private Health Investments? Malaria Control Policies in Eritrea<sup>\*</sup>**

It is often argued that engaging in indoor residual spraying (IRS) in areas with high coverage of mosquito bed nets may discourage net ownership and use. This is just a case of a public program inducing perverse incentives. We analyze new data from a randomized control trial conducted in Eritrea which surprisingly shows the opposite: IRS encouraged net acquisition and use. Our evidence points to the role of imperfect information. The introduction of IRS may have made the problem of malaria more salient, leading to a change in beliefs about its importance and to an increase in private health investments.

JEL Classification: D12, D83, H42, I10, I12

Keywords: malaria, bed nets, indoor residual spray, information, beliefs, behavior, crowding out, health, developing countries

Corresponding author:

Pedro Carneiro  
Department of Economics  
University College London  
Gower Street  
London WC1E 6BT  
United Kingdom  
E-mail: [p.carneiro@ucl.ac.uk](mailto:p.carneiro@ucl.ac.uk)

---

<sup>\*</sup> We would like to thank officials at the Eritrean Ministry of Health and the National Malaria Control Program, and the data collection team at the Orotta School of Medicine in Asmara. Without them this research would not have been possible. This research was funded by a World Bank grant (as part of the HAMSET II project) through the Project Management Unit of the Eritrea Ministry of Health, and a World Bank grant from DIME. We thank Pascaline Dupas, Costas Meghir, Petra Todd, and seminar participants at UCL, EDePo at IFS, the AEL Conference 2011, the 10th Arnoldshain Seminar, the World Bank, PSE and the NEUDC Conference 2011 for helpful comments. Carneiro gratefully acknowledges the financial support from the Economic and Social Research Council for the ESRC Centre for Microdata Methods and Practice (grant reference RES-589-28-0001), the support of the European Research Council through ERC-2009-StG-240910-ROMETA and Orazio Attanasio's ERC-2009 Advanced Grant 249612 "Exiting Long Run Poverty: The Determinants of Asset Accumulation in Developing Countries", and the hospitality of the World Bank Research Group.

Most public programs induce behavioral responses in their target population. These responses are often perverse, making these programs less effective than what was originally intended. For example, the success of public health programs is limited by (among other things) the extent to which they crowd out private health investments. This is a central concern in the design of public interventions across a variety of areas, in rich and poor countries alike. In the particular case of malaria control programs, such as indoor residual spraying (IRS),<sup>1</sup> the introduction of IRS could have a negative impact on the acceptability of insecticide treated mosquito bed nets (ITN), possibly inducing individuals to stop using them (see, e.g., Lengeler (2011)).

In the standard model, the amount of crowding-out depends on the degree of substitutability between private and public investments. However, outside the scope of this simple model are situations where the introduction of a program conveys new information about the returns to private health investments. For example, the introduction of a new health program in a community can be perceived by its members as an indication that (the government knows that) a particular health problem has become more serious in the community, inducing a change in the beliefs about the returns to private health investments (i.e., a program may have an implicit information component even when it does not include an explicit information campaign). In this context, the standard crowd-out intuition breaks down, and an increase in public health investments can lead to an increase in private health investments even when they are substitutes.

Although this is a fairly sensible point, and potentially relevant for most education and health programs in developing countries, it is absent from the discussion on the behavioral responses to such programs. This paper presents experimental evidence from Eritrea that an IRS campaign led to increases in ITN ownership and use. Our analysis suggests that the introduction of IRS may have made the problem of malaria more salient in treatment villages, leading to a change in beliefs about

---

<sup>1</sup>IRS consists in spraying the interior walls of dwellings with insecticide to kill resting mosquitoes.

the importance of the disease in these areas, which resulted in an increase in private health investments.

The data used in our study come from an experimental evaluation of the impact of IRS in the most malarious region of Eritrea (Gash Barka), organized by the Government of Eritrea. Fifty-eight (58) villages were randomly assigned to treatment and 58 villages were randomly assigned to control. Between June–July 2009, before the start of the malaria season, households in treatment villages were visited by government workers carrying IRS equipment and were offered free IRS. Households in control villages did not receive publicly provided IRS and IRS is not privately provided in the market. A household survey and rapid diagnostic tests (RDT) were administered during the malaria season that followed (October, 2009).

Our data show that IRS had no detectable impact on (the already very low levels of) malaria parasite infection prevalence (Keating, Locatelli, Gebremichael, Gebremeskel, Mufunda, Mihreteab, Berhane, and Carneiro (2011)). However, it led to higher ownership and use of ITNs. In addition, households in treatment villages are more aware of (and concerned with) malaria than in control villages. In particular, they are more likely to mention mosquitoes as a malaria vector and to mention children as one of the groups most affected by malaria.

A large literature debates the extent to which a variety of public programs discourages (or crowds-out) private investments in those goods or services which are provided by the public sector. Three examples (among many) are Peltzman (1973), who discusses the case of higher education in the US, Cutler and Gruber (1996), who study health insurance in the US, and Das, Dercon, Habyarimana, Krishnan, Muralidharan, and Sundararaman (2011), who analyze education subsidies in Zambia and India. Examples of the importance of crowding-out effects for health programs in developing countries are much less common in the literature, perhaps because of lack of data. A recent survey of the literature barely mentions this issue (Dupas (2011)).

The standard presumption in these papers is that there is substitutability between private and public expenditures, say, in health, and that individuals have perfect in-

formation about the returns to their health investments. There is however increasing evidence that decision making by the poor is greatly affected by limited information (e.g., Bertrand, Mullainathan, and Shafir (2006), Banerjee and Duflo (2011) and Dupas (2011)). This means that health programs have the potential to simultaneously deliver health services and induce changes in beliefs about the returns to health investments in the populations they serve, which could even lead to a reversal of potential crowding-out effects.

Beyond the literature on crowding-out effects of public programs, it is also important to mention how our study fits into the literature on malaria control programs, and on information and health in developing countries. We contribute to the understanding of ITN use, which is the main tool available to households to prevent infection. Several studies have investigated ways to promote acquisition and usage of ITNs in malarious villages and attention has been focused on the comparison between free-distribution and cost-sharing programs. One central paper on this topic is that by Cohen and Dupas (2010), who provide evidence in support of free distribution.

Providing information about the returns from using a technology can also be an effective way to promote both take-up and use. Dupas (2011) reviews several studies that show how the provision of information can effectively influence people's health-seeking behavior, when they are not already fully informed about the health situation they face, when the source of information is credible and when they are able to process this new information.

In a study of HIV in Malawi, De Paula, Shapira, and Todd (2011) highlight that policies may affect people's behavior if they are able to change their beliefs. They do not find strong evidence that HIV testing consistently affects people's beliefs about their own HIV status (see also Delavande and Kohler (2009)). They also show that downward revisions in beliefs about HIV status increase risky behavior, while the opposite occurs with upward revisions.

Borrowing from the literature in marketing and psychology, Dupas (2009) analyzes how the framing of information on the benefits of ITN use affects ownership

and use of ITNs. She compares two cases: one which stresses the financial gains from a reduction in missed work and another highlighting the health gains from avoiding malaria. Using data from a randomized control trial (RCT) from Kenya, Dupas finds that neither take-up nor usage are affected by how benefits are framed in a marketing campaign. As a possible explanation, she proposes that the stakes are high and that liquidity constraints are probably the main barrier to investments in malaria prevention.

This aspect is further investigated by Tarozi, Mahajan, Blackburn, Kopf, Krishnan, and Yoong (2011), who conducted a RCT in India, to estimate the effectiveness of micro-loans in promoting ITN ownership and use, to reduce malaria prevalence. Their intervention was effective in promoting ITN ownership and use, but had no impact on malaria prevalence. Tarozi et al. (2011) rule out that the intervention caused any “perverse” behavioral response. In other words, their results showed no reduction in any pre-existing anti-malaria behavior. If anything, such behaviors actually increased in treated groups. The authors do not explain this phenomenon, but it is possible that the mechanism that we emphasize in our paper is also at work in theirs.

The remainder of the paper is organized as follows. In Section 1 we briefly describe the study area and the status quo in malaria eradication. In Section 2 we describe our dataset and we introduce our model in Section 3. We present and discuss our estimates in Section 4. Section 5 concludes.

## **1 IRS in Eritrea and the Intervention**

Eritrea has an estimated population of 3.6 million. Malaria dramatically declined in the country over the past decade, from a national peak of 260,000 clinical cases diagnosed in 1998 to just under 26,000 cases in 2008. More than half of all diagnosed malaria cases and over 60 percent of all related deaths in the country come from Gash Barka Zone (2007, 2008), where this study was conducted.<sup>2</sup>

---

<sup>2</sup>We present maps of the study area in Figures 1 and 2 in Appendix 4.



Malaria is transmitted, mainly at night, from infected to healthy people, by female *Anopheles* mosquitoes. Three main technologies are currently used to reduce transmission: ITNs, larval habitat management (LHM) and IRS. ITNs must be hung over the bed at night to protect sleeping individuals from infectious mosquito bites; LHM includes activities such as draining stagnant water, to destroy the habitat of mosquitoes; IRS consists in spraying the inside walls of dwellings with insecticide to kill resting mosquitoes.

The costs of IRS are borne by the Government, which is in charge of conducting spray campaigns. In contrast, ITNs must first be acquired by individuals and then set up above the bed. Sleeping under a net is perceived as unpleasant, especially in warm weather. ITNs also need regular re-impregnation, if they are not coated with long lasting insecticide. LHM campaigns are carried out by the Government with the active involvement of local populations.

Eritrea has been successful in greatly reducing malaria prevalence,<sup>3</sup> however elimination has not yet been achieved. Complete malaria eradication is therefore a priority in Eritrea. Accordingly, the National Malaria Control Program (NMCP) is currently developing strategies to reduce the infection rate to zero.

IRS is an expensive intervention, although generally perceived as effective. Nevertheless, there are no studies of the added benefit of IRS in low-transmission settings over and above ITN use, effective case management, and LHM. As such, the NMCP conducted an evaluation of the impact of IRS in the context of the existing control program (which promotes LHM and ITN use), with the support of the World Bank. The results of this evaluation are presented in Keating et al. (2011).

A two-arm cluster-randomized community-controlled trial, post-test only design was used to evaluate the impact of IRS on malaria infection prevalence. Effectiveness was measured as a single difference between treatment and control groups.

One hundred and sixteen (116) villages in Gash Barka (perceived as especially malarious) were selected for the study. Fifty-eight (58) villages were randomly assigned to the treatment group and 58 villages were randomly assigned to serve

---

<sup>3</sup>See Figure 4 in Appendix 4.

as the control group. A geographic buffer was used to insure that treatment and control villages were at least 5 km apart.<sup>4</sup> The NMCP verified the distance between treatment and control villages, and villages that were too close (less than 5 km apart) to another were replaced by the closest village, at least 5 km apart. In addition, further replacements were made in a few cases where the originally chosen village had moved and could not be found or reached. Again, the closest eligible village was chosen as a replacement. This procedure is discussed in more detail in the next section and village replacements are documented in detail in Appendix 5.

The intervention involved the control of adult mosquito populations using IRS with the insecticide dichlorodiphenyltrichloroethane (DDT), which is recommended by the Eritrean NMCP. In each intervention village, dwellings were sprayed according to the manufacturer's recommended guidelines. The spraying targeted all households to ensure a minimum coverage of 80 percent, as recommended by the World Health Organization's (WHO). Spraying was done during the months of June–July 2009. Treatment and control villages received similar levels of ITNs, LHM and case management, per existing NMCP guidelines and policy. Further details on the study design and intervention are available in Keating et al. (2011).

## 2 Data

A household survey was conducted in October 2009 (a baseline survey was not collected because of budgetary constraints). This corresponds to the period just after the peak of the malaria season. Only one person per household was interviewed and the response rate was high at 94.23 percent, yielding a total sample size of 1,617 households (corresponding to 7,895 individuals), of which 809 lived in treatment villages and 808 resided in control villages. All present and consenting household members were tested for malaria using Carestart® RDTs.<sup>5</sup> Microscopy was used

---

<sup>4</sup>The 5 km threshold was set to ensure that control villages could not benefit from the intervention conducted in treatment villages.

<sup>5</sup>A total of 5,502 people were tested with RDT. 1,120 people were absent at the time of the survey and they could not be tested. In addition, 651 people refused testing. Among those tested,

to validate positive RDT results. Appendix 2 provides a detailed description of the data and of all the variables used in this paper.

Tables 1 and 2 present means and standard deviations for variables which are essentially pre-determined, and mean differences in these variables between the treatment and the control groups.<sup>6</sup> Table 1 shows individual variables and table 2 shows household variables. The characteristics of treatment and control villages are balanced with one exception: the Tigre tribe is over represented in the treatment group. We take this into account in our analysis by including in all regressions an indicator variable which takes a value equal to 1 if household  $i$  belongs to the Tigre tribe, and 0 otherwise (the exclusion of this variable does not affect our results).

These tables also show joint tests that check the balance of several variables simultaneously. We consider three different sets of variables: those available for the whole sample, those available for respondents only, and those available only at the household level. To conduct the test we run probit regressions of treatment assignment on the variables in each group, and we test whether the coefficients in the regressions are jointly equal to zero. To be precise, let  $Treatment_i$  denote an indicator that takes value 1 if household  $i$  belongs to a treatment village, and 0 otherwise. Let  $X_i$  be a vector of variables in each group. Then we estimate:

$$\Pr(T_i = 1|X) = \Phi(X\beta)$$

where  $\Phi$  is the cumulative density function of the standard normal, and we test whether  $\beta = 0$  (where  $\beta$  is the vector of coefficients associated with each variable). Standard errors are clustered at the level of the community. We do not reject the null hypothesis for any of the three groups of variables, which means that we do not

---

13 individuals tested positive in the control group and 17 tested positive in the treatment group. The difference between the share of positive RDTs in the two groups is 0.001 (st. err. = 0.003) and not significant (see Keating et al. (2011)). Malaria prevalence was very low in the area under investigation. More details are presented in Section 3.1 of Appendix 3.

<sup>6</sup>Even though some of these variables could potentially respond to the intervention, it is highly unlikely that any response took place between the time of the intervention (June–July, 2009) and the time of the survey (October, 2009).

reject that these variables are jointly equal in the treatment and control groups. This provides additional evidence that randomization was effective in achieving balance in the characteristics of treatment and control villages.<sup>7</sup>

Half the population in our sample consists of females, as shown in table 1. Almost all household members usually live in the house visited by the interviewer. The population is quite young, with the average age only at 22, and the average age of respondents is about 42. Average levels of education in our sample are low: only 19 percent of respondents ever attended school and 76 percent of them attended only primary school. The proportion of literate respondents is equally low, at 19 percent. Almost all respondents are muslim and married.

Table 2 shows that average household size in the sample is between 4 and 5, with more than half of household members being below 18 years of age. Respondents living in these villages are very poor: only 43 percent of them has access to drinking water from a public tap, 6 percent has a toilet, 25 percent owns a radio, 95 percent uses firewood as the main source of fuel and the average number of rooms per house is well below 2.

Table 3 shows that there was high but not perfect compliance with treatment. Our data shows that 6 percent of households living in control villages reported having their dwelling sprayed in the 5 months prior to the survey.<sup>8</sup> This spraying was not done by the government. Most likely, households used simple insecticide sprays purchased from local shops, which have low effectiveness when compared to IRS.<sup>9</sup> Also, 25 percent of households in treatment villages reported not receiving

---

<sup>7</sup>As mentioned above, the list we originally used to randomly assign villages to treatment or control included 116 villages. Some names were changed at the time of the intervention or when the data collection was conducted, and some villages had to be replaced because they were not found. A very detailed analysis of this issue is presented in Appendix 5, along with robustness checks. Our analysis makes us confident that randomization was indeed effective.

<sup>8</sup>This is roughly the period of time between treatment and the interviews, allowing for some recall error.

<sup>9</sup>Respondents were asked whether anyone had sprayed the interior walls of their dwelling against mosquitoes, at any time over the previous 12 months. NMCP records report that no IRS campaigns was conducted in control villages over the 12 months to the survey. We can also exclude that some other organization conducted an IRS campaign in the region. So, because the question did not

IRS. This may have occurred because all household members were absent at the time of the intervention, or because the residents did not authorize spraying inside their home.<sup>10</sup>

Throughout the paper we report simple comparisons between treatment and control communities. Given that compliance with spraying was not perfect, one may think of also reporting instrumental variable estimates of the impact of the program on various outcomes, where each household’s participation in spraying is instrumented by the community level treatment indicator. Estimates are reported in tables 12–16 in Section 4 of Appendix 3. We notice that these estimates are very similar to those presented in tables 5–9 in the paper. The reason why we focus on the community level treatment variable in the main text is that the intervention is likely to affect the beliefs and behaviors of all residents in the community. Given that spraying was so widespread in each community it will be visible to everyone, not only to those who actually received spraying.

### 3 Theoretical Framework

To guide our empirical analysis we present a simple model of behavioral response to the introduction of IRS under perfect and imperfect information about the probability of malaria infection. The proofs of the results presented in this section are reported in Appendix 1.

There are  $N$  identical workers, indexed by  $i = 1, 2, \dots, N$ , and each worker has the same time endowment,  $time_i = T, \forall i$ . All individuals work (labor supply is inelastic) at wage  $w$ , which is exogenously determined. Mosquitoes are infected

---

specify “with DDT” or “by spraying teams”, these respondents may have plausibly answered yes if they had engaged in personal spraying with commercially bought insect repellent to coat their walls. The effect of such sprays is very limited compared to that of DDT.

<sup>10</sup>Participation was voluntary, so some households may have not allowed IRS in their homes. In addition, there may have been lack of sufficient insecticide to treat all houses, and some dwellings maybe have been located very far from the center of the village so they were not reached by the IRS campaign. As we mentioned above, spraying targets all households to guarantee that at least 80% of the village is covered (WHO guidelines), so some degree of imperfect compliance was expected.

with malaria, and malaria affects the time endowment of worker  $i$  by reducing the time available to him from  $T$  to  $T - t, t > 0$ . The probability that an infected mosquito finds a worker  $i$  is  $\pi_i \equiv \pi \geq 0, \forall i$ . Mosquitoes bite and infect all the workers they find, unless workers use some malaria preventive technology.

For simplicity, there are only two available technologies to protect workers from malaria: ITNs and IRS. In the following, we refer to ITNs and IRS as  $\Phi$  and  $\Psi$  respectively. Technology  $\Phi$ , ITNs, is available to all workers, and it can protect them from infected mosquitoes with probability  $p^\Phi \in (0, 1)$ , preventing a reduction in their time endowment. Adoption of  $\Phi$  causes disutility to the worker ( $d_i > 0$ ),<sup>11</sup> so some workers may decide not to use it. Technology  $\Psi$ , IRS, can protect them from infectious bites with probability  $p^\Psi \in (0, 1)$ , preventing a reduction in time endowment. Use of  $\Psi$  does not entail any disutility for workers. Therefore, all workers will choose to use  $\Psi$ , if it is made available to them.

Suppose technology  $\Phi$  is available to all who want it. Technology  $\Psi$  may be introduced on top of  $\Phi$  in an attempt to grant workers additional protection from malaria and allow them to work as much as possible. We assume that using two technologies jointly offers more protection than using either alone:<sup>12</sup>

**Assumption 1.**  $\max(p^\Phi, p^\Psi) < p^{\Phi \cup \Psi}$

where  $p^{\Phi \cup \Psi}$  is the probability that a worker is protected from infectious bites, if he uses both technologies. Workers are risk neutral, with utility function  $U_i = Y_i - \phi_i d_i$ , where  $\phi_i$  is a dummy variable equal to 1 if worker  $i$  chooses to use  $\Phi$  and 0 otherwise, and  $d_i$  represents an idiosyncratic disutility incurred when using technology  $\Phi$ . Each worker chooses whether to use  $\Phi$ , to maximize his own expected utility:

---

<sup>11</sup>Disutility may arise from a variety of factors that negatively impact ITN users, including: the need to hang the net over the bed every night; sleeping closer to other household members to fit more people inside a net; a reduction in ventilation during the hours of sleep; and allergic reactions caused by contact with the insecticide on the ITN.

<sup>12</sup>This seems a reasonable assumption, in light of the evidence presented in Kleinschmidt, Schwabe, Shiva, Segura, Sima, Mabunda, and Coleman (2009) that combined use of IRS and ITNs reduces the probability of malaria infection more than use of either technology alone. They show that the protective efficacy of either technology is unaffected by the use of the other.

$$\phi_i^* \in \arg \max_{\phi_i \in \{0,1\}} E(U_i | \Psi) \quad (1)$$

In this simple model, we do not account for any externalities which may arise from others' use of ITNs. Even though they are potentially important, our main point can be made without mentioning them.<sup>13</sup>

### 3.1 Perfect Information

Under perfect information, all workers know that the probability of infectious bites,  $\pi$ , is  $\bar{\pi} > 0$ . If  $\Psi$  is not introduced, the expected time endowment for each worker is:

$$\begin{aligned} E(time_i) &= (1 - \bar{\pi})T + \bar{\pi} \{ (1 - \phi_i)(T - t) + \phi_i[p^\Phi T + (1 - p^\Phi)(T - t)] \} \\ &= T - \bar{\pi}t(1 - \phi_i p^\Phi) \end{aligned} \quad (2)$$

where  $\phi_i$  is an indicator variable which takes value 1 if the individual uses an ITN and takes value 0 otherwise. If no mosquitoes find and infect worker  $i$ ,<sup>14</sup> he will have full time endowment  $T$  irrespective of his use of  $\Phi$ . If a mosquito finds him (with probability  $\bar{\pi}$ ) and if he does not sleep under an ITN, he will lose time endowment  $t$ , and will be left with  $T - t$ . ITN use would grant him protection with probability  $p^\Phi$ , preventing him from losing  $t$ .

Worker  $i$  will use technology  $\Phi$  if its use can increase his expected utility relatively to the case in which he does not use it. This happens if the expected gains from ITN use compensate the disutility incurred from its use:

---

<sup>13</sup>A discussion is presented in Section 5 of Appendix 1.

<sup>14</sup>We assume that mosquitoes infect with certainty all workers they find.

$$\begin{aligned}
\phi_i^* = 1 &\Leftrightarrow E(U_i|\phi_i = 1) > E(U_i|\phi_i = 0) \\
&\Leftrightarrow w(T - \bar{\pi}t + \bar{\pi}p^\Phi t) - d_i > w(T - \bar{\pi}t) \\
&\Leftrightarrow w\bar{\pi}tp^\Phi > d_i
\end{aligned} \tag{3}$$

The provider of preventive technologies, i.e., the government, may decide to provide  $\Psi$ . In that case:

$$E(\text{time}_i|\Psi = 0) = T - \bar{\pi}t(1 - \phi_i p^\Phi) \tag{4}$$

$$E(\text{time}_i|\Psi = 1) = T - \bar{\pi}t[1 - (p^\Psi)^{1-\phi_i}(p^{\Phi \cup \Psi})^{\phi_i}] \tag{5}$$

$$\text{If } \Psi = 0, \text{ then } \phi_i^* = 1 \Leftrightarrow w\bar{\pi}tp^\Phi > d_i \tag{6}$$

$$\text{If } \Psi = 1, \text{ then } \phi_i^* = 1 \Leftrightarrow w\bar{\pi}t(p^{\Phi \cup \Psi} - p^\Psi) > d_i \tag{7}$$

Expressions (4) and (6) are identical to (2) and (3). Equation (5) shows how the probability of infection is affected by the introduction of  $\Psi$ . Condition (7) shows that, once spraying campaigns have been rolled out, workers will choose to sleep under an ITN if the *additional* expected gains from its use can compensate for the associated disutility.

We are interested in understanding how the introduction of IRS affects average ITN use. Let  $\theta^\Phi \equiv E(\phi_i^*|\Psi = 0)$  be the average use of  $\Phi$  when  $\Psi$  is not introduced, and let  $\theta^\Psi \equiv E(\phi_i^*|\Psi = 1)$  represent the same measure if  $\Psi$  is made available. The difference in average ITN use is governed by the relationship between conditions (6) and (7). This comparison requires an *rn v* assumption on the degree of complementarity between  $\Phi$  and  $\Psi$ . It is reasonable to start by assuming that  $\Phi$  and  $\Psi$  are substitutes, i.e.,  $p^{\Phi \cup \Psi} \leq p^\Phi + p^\Psi$ . In this scenario (with perfect information) we show in Appendix 1 that the average use of  $\Phi$  cannot increase following the introduction of  $\Psi$ :  $\theta^\Psi \leq \theta^\Phi$  (this is because no worker who does not use  $\Phi$  in the absence of  $\Psi$ , would start using  $\Phi$  in the presence of  $\Psi$ ). It is plausible, but less



natural, that the two technologies are complements instead, i.e.,  $p^{\Phi \cup \Psi} \geq p^\Phi + p^\Psi$ . In this case the opposite is true:  $\theta^\Psi \geq \theta^\Phi$ .

### 3.2 Imperfect Information

In a more realistic setting, workers do not know the true value of  $\pi$ . Suppose that  $\pi$  can only take one of two values: 0 or  $\bar{\pi} > 0$ .<sup>15</sup> Each worker  $i$  is endowed with a prior  $P_i(\pi = \bar{\pi})$  (and  $P_i(\pi = 0) = 1 - P_i(\pi = \bar{\pi})$ ) drawn from a  $Uniform(0, 1)$ . Workers believe that the provider of  $\Psi$  has perfect knowledge about  $\pi$ . The mapping between the government's decision to spray and  $\pi$  is not deterministic, i.e., the government does not always spray when  $\pi$  is high (say, because of resource constraints), and it may spray in some cases where  $\pi$  is zero (say, either because of different information, or as a preventive measure). Our assumption is that individuals believe that the probability that the government sprays when the true risk of infection is 0 cannot exceed the probability that it does so when malaria poses a threat:<sup>16</sup>

**Assumption 2.**  $\Pr(\Psi = 1 | \pi = \bar{\pi}) \geq \Pr(\Psi = 1 | \pi = 0)$ .

Workers update their beliefs using Bayes' rule after observing the realization of  $\Psi$ . We can compute expressions (8) and (9) for the expected time endowment, which are analogous to (4) and (5):

$$E(time_i | \Psi = 0) = T - P_i(\pi = \bar{\pi} | \Psi = 0) \bar{\pi} t (1 - \phi_i p^\Phi) \quad (8)$$

$$E(time_i | \Psi = 1) = T - P_i(\pi = \bar{\pi} | \Psi = 1) \bar{\pi} t [1 - (p^\Psi)^{1-\phi_i} (p^{\Phi \cup \Psi})^{\phi_i}] \quad (9)$$

Expression (8) is identical to (4), except for the fact that the posterior probability

---

<sup>15</sup>This formulation simplifies the structure of the problem, still capturing its essence, and it seems suitable to study the very low transmission environment under investigation.

<sup>16</sup>People are aware that the government has successfully managed to drastically reduce malaria in recent years, and so they understand that it is committed to fight the disease. This makes the government "credible".

of infection is now multiplying the expected time savings, and similarly for equation (9). We can use these two equations to obtain conditions (10) and (11) for ITN use, depending on the availability of  $\Psi$ :

$$\text{If } \Psi = 0 \text{ then } \phi_i^* = 1 \Leftrightarrow P_i(\pi = \bar{\pi} | \Psi = 0) w \bar{\pi} t p^\Phi > d_i \quad (10)$$

$$\text{If } \Psi = 1 \text{ then } \phi_i^* = 1 \Leftrightarrow P_i(\pi = \bar{\pi} | \Psi = 1) w \bar{\pi} t (p^{\Phi \cup \Psi} - p^\Psi) > d_i \quad (11)$$

Again, we want to understand how the introduction of IRS may affect average ITN use, and we can do this by comparing conditions (10) and (11). As before, the relationship between  $(p^{\Phi \cup \Psi} - p^\Psi)$  and  $p^\Phi$  depends on whether the two technologies are substitutes or complements, but now the expected gains from ITN use also depend on the posterior probabilities of infection. Assumption 2 implies that  $P_i(\pi = \bar{\pi} | \Psi = 1) \geq P_i(\pi = \bar{\pi} | \Psi = 0)$ . Therefore, with imperfect information,  $\theta^\Psi$  could be either larger or smaller than  $\theta^\Phi$ , even when  $\Phi$  and  $\Psi$  are substitutes. This is in contrast with the analogous result for the perfect information case, for which the result was unambiguous.

Finally, if agents perceive  $\Phi$  and  $\Psi$  to be complements, it is easy to show that the average use of ITNs may either remain unchanged or increase with the introduction of  $\Psi$ , as in the previous scenario. Table 4 summarizes the predictions of the model, under either assumption.

## 4 Data Analysis

### 4.1 Basic Results

In this section we analyze the impact of the IRS campaign on a set of behavioral and socio-economic outcomes. In particular, we look at the effect of spraying (1) on the level of information and awareness of malaria among the people of Gash Barka, (2) on the ownership and use of mosquito bed nets, as well as (3) on their

intra-household allocation. The impact of IRS on the prevalence of malaria was found to be zero in our earlier work (see Keating et al. (2011); see also tables 1 and 2 in Appendix 3).

In tables 5–8 we compare treatment and control villages across a variety of dimensions (information and knowledge of malaria, ownership and use of mosquito bed nets, participation in LHM, and behaviors conducive to malaria eradication other than LHM). The first two columns of each table present means and standard deviations for each variable, for treatment and control villages. The remaining columns report differences (and corresponding standard errors) between treatment and control villages using three different specifications (which, given our experimental design, we interpret as the impact of the program). The first specification does not account for any control variables, and therefore corresponds to a simple difference in means between the two sets of villages. The second and third specifications include, respectively, a very simple set of control variables (dummy indicating whether an individual belongs to the Tigre tribe,<sup>17</sup> a dummy indicating Muslim religion, and dummies for subzone of residence), and a more complete set of control variables which includes all the variables we analyzed in the randomization checks (which we call  $X_{other}$  in the equations below).<sup>18</sup> We estimate the program impact using least squares regression (12) of  $Y$  on a treatment indicator ( $Treatment$ , in the equation below) and control variables when  $Y$  is a continuous variable, or using probit model (13) when  $Y$  is binary (marginal effects are presented in this case):

$$Y = \alpha + \beta Treatment + \gamma_1 Tigre Tribe + \gamma_2 Muslim + \gamma' Subzones + X_{other}\lambda + \epsilon \quad (12)$$

$$\Pr(Y = 1) = \Phi(\alpha + \beta Treatment + \gamma_1 Tigre Tribe + \gamma_2 Muslim + \gamma' Subzones + X_{other}\lambda) \quad (13)$$

---

<sup>17</sup>This is the main tribe in Gash Barka and it is over-represented in treatment villages.

<sup>18</sup>School enrolment is excluded because it is recorded only for children in school age.

where  $\Phi$  is the cumulative density function of the standard normal. Standard errors are clustered at the village level. Across tables, our estimates are almost identical for models with different controls (columns 3–5). Much of our discussion will focus on the specification with basic regressors.

Table 5 shows that, in spite of the fairly low levels of parasite prevalence in the region,<sup>19</sup> malaria is still (correctly) perceived as a problem in the community by a large majority of the population, both in treatment and control villages. However, we notice that more than 25 percent of respondents report that malaria is not a problem in their community (despite the fact that our survey was conducted in the most malarious villages in Eritrea).<sup>20</sup> There is also widespread knowledge that mosquitoes are an important transmission vector. Even though almost everyone agrees that children are especially at risk from malaria, only about a third of respondents believe that pregnant women suffer greatly from having malaria. Finally, about half of the respondents were aware of information campaigns conducted during the 6 months prior to the interview, concerning ITNs, early seeking behavior (seeking timely treatment and proper diagnostic of malaria symptoms) and environmental management.

Table 5 also presents the estimated effect of the IRS campaign on information and knowledge about malaria. Our estimates suggest that treatment increased knowledge that mosquitoes are a vector by about 3 percent, and awareness that children are especially at risk from malaria by almost 7 percent. On average, respondents did not become more worried that malaria was a problem in their community, nor that women are particularly vulnerable to malaria. We test and reject that these four variables are jointly equal in treatment and control villages. These results show there is more concern with malaria transmission in treatment than in

---

<sup>19</sup>Keating et al. (2011) document a prevalence rate below 1 percent (October, 2009).

<sup>20</sup>The Global Malaria Action Plan of the Roll Back Malaria initiative (available at <http://www.rbm.who.int/gmap/>) explains that the situation whereby villagers lose interest in malaria and in prevention, in areas where malaria has been dramatically reduced by successful control efforts, is referred to as “malaria fatigue”, and that it can lead the public to reduce use of the available preventive and treatment measures. So this issue must be addressed properly and in a timely fashion.

control villages, suggesting that the provision of IRS led individuals to update their beliefs about the importance of malaria in their communities. In particular, the increased concern with the impact on malaria on children, paired with an increased awareness that mosquitoes are the transmission vector for the disease, may have changed the expected returns to malaria prevention behaviors such as ITN use.

It is also useful to notice that respondents in treatment villages did not receive significantly more information on ITNs, early seeking behavior and environmental management over the previous 6 months, than those in the control group. These variables are not statistically different in treatment and control variables, either when we look at them individually or jointly. This suggests that any changes in information and knowledge are a direct consequence of the IRS campaign.

Table 6 reports information on ownership and use of bed nets. In this Section we draw a distinction between “ITNs” and “nets”: we restrict the former definition to consist only of those nets that were properly treated with insecticide at the time of the survey,<sup>21</sup> while we use the latter term to additionally include those nets that had not been properly re-treated. On average, there were about 1.58 nets and 1.28 ITNs per household in the control group villages. Furthermore, an average of 1.16 nets per household were used the previous night and 0.736 nets were left unused. These figures are slightly higher in the treatment villages. A comparison of ownership figures for any nets versus ITNs suggests that the vast majority of owned bed nets were properly insecticide treated at the time of the survey.<sup>22</sup> About 40 percent of all household members in control villages reportedly slept under a net (net use) the night before the survey.

---

<sup>21</sup>We include in the definition of “ITNs”: all Long Lasting Insecticide treated Nets (LLINs), which were distributed in the area starting from 2006 and whose insecticide is effective for 3–5 years; all ITNs acquired in the 3 years prior to the survey (which are most likely LLINs, since the government distributed exclusively LLINs since 2006); and all ITNs that were re-treated in the 12 months before the survey, in accordance with NMCP guidelines.

<sup>22</sup>We do not study explicitly households’ participation in net re-impregnation activities because LLINs have progressively replaced traditional ITNs since the NMCP discontinued distribution of the latter in favor of the former in 2006. An additional reason for omitting an analysis of re-impregnation behavior is that we include in the definition of LLINs also all ITNs acquired in the 3 years before the survey (as we explained in footnote 21) and LLINs need not be re-impregnated.

In table 6 we also present the estimated program effects on ownership and use of bed nets.<sup>23</sup> Households living in treated villages own 0.214 more nets and 0.176 more ITNs than households from control villages. The number of nets used the night before the survey was 0.186 higher in treated villages, but there was no discernible difference in the number of unused nets between treatment and control. We jointly test and reject (at the 10 percent level of significance) that there is no difference in these four variables between treatment and control villages. The proportion of individuals reported to have used a net is higher in treatment than in control villages but the difference is not statistically strong (this variable is not included in the joint test because it is an individual rather than a household variable). These results show a clear difference in net ownership and use between treatment and control villages.

Our results are consistent with the model we developed in Section 3. In response to the introduction of IRS in a community, its inhabitants experience an increase in awareness and concern about malaria (especially about the danger of mosquito bites), which affects their ownership and use of ITNs. As far as we know, this mechanism has not been discussed before in the literature, although it could be important in many settings. By introducing a program in a community, be it a health, education, or other type of program, a government potentially provides information about its knowledge of the problem addressed by the program, or it just makes the problem more salient in the minds of community members. When in-

---

<sup>23</sup>An interesting question is whether households can (and do) acquire new bed nets if they want to do so, or whether supply is determined solely by free distribution campaigns that provide the same number of nets to every household. To shed some light on this point, we use an asset index (described in Section 3.2 of Appendix 3) to compare statistics on ownership of bed nets by wealth quintile. Focusing on the control group (i.e., in the absence of the intervention), we see that net ownership increases with wealth, so that households in the top quintile own a number of nets (2.17) which is about double that of households in the lowest quintile (1.24). The same can be said about ITNs: ownership increases progressively over wealth quintiles, from 0.99 to 1.59 ITNs per household. This is suggestive that ownership of bed nets is not exogenously determined by free distribution campaigns. To the contrary, wealthier households can and do obtain a larger number of nets. They may do so, e.g., by purchasing nets from a local market or from poorer households, or they may possibly exploit their bargaining power to obtain more free nets during distribution campaigns.

dividuals have imperfect information and face uncertainty about the importance of the particular problem at hand, such revelation of information may lead individuals to update their beliefs and, as a result, change their behaviors. These changes in behaviors are generally not expected by those designing the program. This section shows that they can be quite important.

In addition to using bed nets, individuals can engage in other preventive behaviors to reduce the risk of malaria infection. For example, they can keep any cattle away from home, cover any stored water and participate in environmental management campaigns, among others. Table 7 focuses on participation in LHM campaigns and it shows that participation is fairly low across a variety of measures, as pointed out in Keating et al. (2011). Table 8, which includes the full range of mentioned ways how respondents try to avoid mosquito bites, shows that households engage in a wide variety of malaria prevention behaviors other than ITN use and LHM.

Tables 7 and 8 also report estimates of the impact of IRS on those behaviors. We do not find evidence that IRS crowded-out private investment in any of those behaviors.<sup>24</sup> If anything, the IRS campaign had a positive effect, especially on the proportion of households who keep their livestock away from their dwelling, which increased by as much as 6.76 percent.<sup>25</sup>

---

<sup>24</sup>Standard errors are rather small in tables 7 and 8, so we would have been able to detect a negative impact of IRS on these sets of behaviors, had there been any. In addition, most coefficients have a positive sign (particularly so in table 7), whereas a negative sign would hint to the presence of crowd-out. The joint test in table 7 omit the third variable in the table (number of household members who participated in LHM) because it is just the sum of the three subsequent variables in the table. Similarly, in table 8 the first two variables are omitted from the joint test only because the sample of non-missing answers for these variables is much smaller than for the remaining variables in the table.

<sup>25</sup>As mentioned above, instrumental variables estimates of the impact of IRS on all these outcomes (where household participation in IRS is instrumented by the village treatment assignment) are presented and discussed in Section 4 of Appendix 3. Our main conclusions are essentially unchanged.

## 4.2 Intra-Household Allocation of Bed Nets

We also checked whether IRS affected net use among some demographic groups and how this changed the intra-household allocation of nets. To do so, we divided the population into six mutually exclusive categories (children under 5 years of age, school age youths (5–20 years old), employed adult (>20 years old) men and women, and unemployed adult men and women) and we analyzed how the intervention affected net use in each of the groups.

Table 3 in Appendix 3 shows that, in the absence of IRS, net usage varies greatly by age, gender and employment status: children under 5 are the most likely to sleep under a bed net (50 percent), followed by unemployed and employed women in working age (44 and 40 percent), school age youths (36 percent) and finally by employed and unemployed adult men (27 and 24 percent). No significant gender differences were observed among children under five or among young people. Among employed adults, women are much more likely to sleep under a bed net (+13 percent) and the same is true among the unemployed (+20 percent).

We estimate the impact of the intervention on the intra-household allocation of bed nets using probit regression (13), letting  $Y$  be a dummy for net use, and restricting the sample in turn to each socio-demographic category. Estimates are presented in table 9 (which shows marginal effects). For each socio-demographic group, the first two columns of table 9 present average bed net use in treatment and control villages, with standard deviations in parentheses. The remaining three columns present the impact of the intervention on the intra-household allocation of bed nets, with the same sets of controls used in tables 5–8 .

Table 9 shows that treatment increased bed net use especially among workers, and we can see in particular that 8 percent more male workers chose to sleep under a bed net; the estimated increase among female workers is not robust to different specifications (and it is not statistically significant from zero in our favorite specification). We notice, importantly, that the use of bed nets did not decline (estimated coefficients are positive but non significant) among children under five, who are



among the most vulnerable to malaria. Similarly, adult women were not negatively affected (irrespective of their employment status).<sup>26</sup>

These results, which show an increase in net use among workers, are consistent with the previous findings that information and awareness about malaria increased in the population, and with the idea that households became more sensitive to the importance of protecting their breadwinners, thereby adapting the intra-household allocation of nets. This evidence is also in line with results presented in Section 3 of Appendix 3, showing that malaria awareness increased especially among workers, who increased net use accordingly. Increased net use among workers may have stemmed from the observed increase in net ownership or from a change in sleeping arrangements, with workers sharing more often sleeping space with their spouse and young children.<sup>27</sup>

Given the estimates in table 5 one could have thought that the largest increase in net use would be among children. However, it is possible that a greater awareness that malaria has a strong impact on children may just be a manifestation of a more general concern and awareness of the dangers of malaria.

---

<sup>26</sup>Adult women include pregnant women, a category that is very vulnerable to malaria. We do not have data about pregnancy.

<sup>27</sup>In Section 3 of Appendix 3 we present additional results describing how the impacts of the program vary with the level of vegetation in the district (“subzoba”) where villages are located, and we focus our attention on the treatment effect on malaria knowledge and on net ownership and use. We also check heterogeneity in impacts according to several characteristics of the respondent (we do not have these information for all household heads, so we use respondents as a proxy; the respondent was the head in 61.71 percent of the households and the spouse in 33.83 percent of the cases): employment status, literacy, education, religion, tribe, female headship, family size, presence of children in the household, and wealth. We see increases in concern with malaria mainly among workers, although we see some increase in information also among the non-employed. This is plausible if information reaches everyone, but if it becomes a source of concern only for workers (because they are the ones who potentially suffer the most if they are afflicted with the disease). Regarding net ownership, we observe that impacts of IRS are much larger for families where the respondent is literate and employed, and they are lower for families in the bottom quintile of the wealth distribution.

## 5 Conclusions

The concern that government intervention crowds-out desirable private behavior is common to several areas of public policy. The standard model predicts that this will happen if private and public inputs are substitutes. This paper emphasizes a new mechanism by which government intervention may encourage a higher provision of the private input, even when private and public inputs are substitutes. This can occur when individuals have little information about the returns to their actions, and when the public intervention reveals information that may lead to an increase in their subjective expectations of the returns to their actions. This is not only interesting, but also likely to be important in a variety of settings. We apply and illustrate the relevance of this idea to the study of a malaria control program in Eritrea.

Several countries in Sub-Saharan Africa, including Eritrea, have successfully reduced the malaria burden in their territory in recent years, using a combination of free ITN distribution, LHM, case management, prompt and effective treatment, and information campaigns. Their governments are now contemplating strategies to eliminate the disease once and for all, and in particular they are considering the introduction of regular IRS campaigns to achieve this goal, whereas IRS was used so far chiefly for emergency response.

Public provision of IRS may crowd-out people's private investment in the existing risk mitigating technologies, possibly leading to a resurgence of the disease rather than to a sharp decrease and its eventual eradication. In a companion paper, we document that a single IRS intervention is not sufficient to eradicate malaria completely in a policy-induced low-transmission setting like the one under investigation. It is therefore of paramount importance that people consistently make use of the preventive technologies available to them, to ensure that malaria eradication can be achieved in the medium run (possibly with the help of several IRS campaigns).

Our main result is that public IRS provision did not crowd-out private investment in any malaria control policy in Eritrea in the short run: in fact, IRS did not

induce a reduction in ownership or use of ITNs, nor did it have a negative impact on any of the other risk mitigating behaviors in which villagers are engaged. If anything, spraying led to an increase in preventive behaviors. We show that IRS increased average ownership of ITNs, and that it promoted net use among workers.

We explain this with a simple model of net use in a setting where individuals have imperfect information about the risk of being infected by a mosquito carrying the malaria parasite, and update their beliefs about the level of malaria prevalence in their area of residence when they observe the introduction of a new intervention. This model proposes that public health interventions may act as marketing campaigns, capable to promote take-up of the existing preventive technologies, and as an information campaign, that fosters active use of the available risk mitigating tools. This can be true even when the original goal of the intervention was neither marketing nor the provision of information, such as in the case of an IRS campaign. Both our empirical results and our interpretation are novel in the literature.

We observe in our data a very high pre-intervention awareness about malaria, about the mode of transmission of the disease, and about who is at increased risk of being ill. We show that IRS provision promoted malaria awareness even further. Mosquito net ownership and use also increased after treatment. This increase in net use occurs mainly among household members who are currently working. We also show that net use among the most vulnerable categories (including children under the age of five and pregnant women) was not negatively affected by the rise in use among workers.

## References

- Banerjee, A. V. and E. Duflo (2011). *Poor Economics: A Radical Rethinking of the Way to Fight Global Poverty*. Public Affairs.
- Bertrand, M., S. Mullainathan, and E. Shafir (2006). Behavioral Economics and Marketing in Aid of Decision Making among the Poor. *Journal of Public Policy & Marketing* 25(1), 8–23.
- Cohen, J. and P. Dupas (2010). Free Distribution or Cost-Sharing? Evidence from a Randomized Malaria Prevention Experiment. *The Quarterly Journal of Economics* 125(1), 1–45.
- Cutler, D. M. and J. Gruber (1996). Does Public Insurance Crowd out Private Insurance? *The Quarterly Journal of Economics* 111(2), 391–430.
- Das, J., S. Dercon, J. Habyarimana, P. Krishnan, K. Muralidharan, and V. Sundararaman (2011). School Inputs, Household Substitution, and Test Scores. *National Bureau of Economic Research (NBER) Working Paper* 16830.
- De Paula, A., G. Shapira, and P. Todd (2011). How Beliefs about HIV Status Affect Risky Behaviors: Evidence from Malawi, Seventh Version. *Penn Institute for Economic Research (PIER) Working Paper* 11-033.
- Delavande, A. and H.-P. Kohler (2009). Subjective Expectations in the Context of HIV/AIDS in Malawi. *Demographic Research* 20, 817–874.
- Dupas, P. (2009). What Matters (and What Does Not) in Households’ Decision to Invest in Malaria Prevention? *American Economic Review* 99(2), 224–30.
- Dupas, P. (2011). Health Behavior in Developing Countries. *Annual Review of Economics* 3(1), 425–449.

- Keating, J., A. Locatelli, A. Gebremichael, T. Ghebremeskel, J. Mufunda, S. Mihreteab, D. Berhane, and P. Carneiro (2011). Evaluating Indoor Residual Spray for Reducing Malaria Infection Prevalence in Eritrea: Results from a Community Randomized Control Trial. *Acta Tropica* 119(2-3), 107–113.
- Kleinschmidt, I., C. Schwabe, M. Shiva, J. L. Segura, V. Sima, S. J. Mabunda, and M. Coleman (2009). Combining Indoor Residual Spraying and Insecticide-Treated Net Interventions. *The American Journal of Tropical Medicine and Hygiene* 81(3), 519–524.
- Lengeler, C. (2011). Optimal Choice of Vector Control Methods. *Presentation at the 3rd Meeting of the Optimal Choice of Vector Control Methods Work Stream, IFRC, Geneva, Switzerland.*
- Peltzman, S. (1973). The Effect of Government Subsidies-in-Kind on Private Expenditures: The Case of Higher Education. *Journal of Political Economy* 81(1), 1–27.
- Tarozzi, A., A. Mahajan, B. Blackburn, D. Kopf, L. Krishnan, and J. Yoong (2011). Micro-Loans, Bednets and Malaria: Evidence from a Randomized Controlled Trial in Orissa (India). *Economic Research Initiatives at Duke (ERID) Working Paper* 104.

Table 1: Randomization checks – Individual Variables

Variables (Y)	Treatment	Control	Difference
ALL HOUSEHOLD MEMBERS			
1- Female	0.52 (0.50)	0.52 (0.50)	-0.0040 (0.0113)
2- Usually lives here	0.98 (0.13)	0.98 (0.16)	0.0062 (0.0049)
3- Stayed here last night	0.97 (0.18)	0.95 (0.21)	0.0137 (0.0086)
4- Age	22.34 (19.52)	22.00 (19.18)	0.3456 (0.4924)
RESPONDENTS ONLY			
5- Age	42.05 (15.01)	41.43 (15.25)	0.6157 (0.8926)
6- Ever attended school	0.19 (0.39)	0.19 (0.39)	0.0072 (0.0339)
7- Only primary school	0.74 (0.44)	0.78 (0.41)	-0.0373 (0.0527)
8- Literate	0.18 (0.39)	0.20 (0.40)	-0.0151 (0.0321)
9- Muslim religion	0.84 (0.37)	0.78 (0.42)	0.0601 (0.0678)
10- Tigre tribe	0.57 (0.50)	0.40 (0.49)	0.1666* (0.0843)
11- Married	0.93 (0.26)	0.94 (0.24)	-0.0125 (0.0133)
P-value [variables 1–4]			0.25
P-value [variables 5–6,8–11]			0.16

Note: for variables 5–11: sample restricted to respondents only. Column (1): sample restricted to treatment group. Column (2): sample restricted to control group. For each variable Y, columns (1) and (2) report means, with standard deviations in parentheses. Column (3) presents the difference between (1) and (2) estimated as follows:  $Y_i = \beta T_i + \varepsilon_i$ , where  $T_i$  is a treatment allocation dummy. Robust standard errors are reported in parentheses. We also use an F-test to check whether groups of controls, with comparable sample sizes, jointly predict treatment and we report the p-values (we run regressions of treatment on different sets of variables). Variable 7 is not used in the joint test because it has missing values for respondents without any schooling, so it has smaller sample size than variables 5–6,8–11. Observations clustered at village level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Table 2: Randomization checks – Household Variables

Variables (Y)	Treatment	Control	Difference
HOUSEHOLD LEVEL VARIABLES			
12- Household size	4.98 (2.30)	4.79 (2.28)	0.1844 (0.1559)
13- Household members under 5	0.85 (0.90)	0.82 (0.94)	0.0214 (0.0566)
14- Household members under 18	2.69 (1.98)	2.60 (1.96)	0.0925 (0.1279)
15- Main source of drinking water			
15a- Public tap	0.42 (0.49)	0.43 (0.50)	-0.0104 (0.0773)
15b- Unprotected well	0.25 (0.43)	0.23 (0.42)	0.0195 (0.0545)
15c- Unprotected spring	0.13 (0.33)	0.14 (0.35)	-0.0150 (0.0384)
16- Has any toilet	0.05 (0.23)	0.07 (0.25)	-0.0112 (0.0232)
17- Has radio	0.25 (0.43)	0.24 (0.43)	0.0084 (0.0324)
18- Firewood is main fuel	0.93 (0.25)	0.96 (0.20)	-0.0214 (0.0185)
19- Has no window	0.32 (0.47)	0.32 (0.47)	0.0050 (0.0656)
20- Number of separate rooms	1.86 (1.18)	1.83 (1.20)	0.0225 (0.1049)
21- Number of sleeping rooms	1.39 (0.82)	1.38 (0.71)	0.0020 (0.0509)
22- Number of sleeping spaces	4.61 (2.45)	4.44 (2.35)	-0.1641 (0.1900)
P-value [variables 12–22]			0.925
P-value [variables 5–6,8–22]			0.276

Note: one observation per household. Column (1): sample restricted to treatment group. Column (2): sample restricted to control group. For each variable Y, columns (1) and (2) report means, with standard deviations in parentheses. Column (3) presents the difference between (1) and (2) estimated as follows:  $Y_i = \beta T_i + \varepsilon_i$ , where  $T_i$  is a treatment allocation dummy. Robust standard errors are reported in parentheses. We also use an F-test to check whether groups of controls, with comparable sample sizes, jointly predict treatment and we report the p-values (we run regressions of treatment on different sets of variables). Variable 7 is not used in the joint test because it has missing values for respondents without any schooling, so it has smaller sample size than variables 5–6,8–22. Observations clustered at village level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Table 3: Compliance with treatment allocation

	Control group	Treatment group
Dwelling was sprayed in past 5 months	49	604
Dwelling was not sprayed in past 5 months	679	124
Missing information or respondent does not know	80	81

Note: This table shows the number of respondents reporting that someone sprayed the interior walls of their dwelling against mosquitoes in the 5 months prior to the survey or that no one did, in the control and in the treatment groups. Five months corresponds approximately to the period of time between the IRS intervention and the survey.

Table 4: Summary of the theoretical predictions

	Imperfect substitutes	Imperfect complements
Perfect Information	$\theta^\Psi \leq \theta^\Phi$	$\theta^\Psi \geq \theta^\Phi$
Imperfect Information	$\theta^\Psi \leq \theta^\Phi$ or $\theta^\Psi \geq \theta^\Phi$	$\theta^\Psi \geq \theta^\Phi$

Note: Average use of  $\Phi$  depending on the complementarity between  $\Phi$  and  $\Psi$  and on the availability of information about malaria prevalence.  $\theta^\Phi \equiv E(\phi_i^* | \Psi = 0)$  and  $\theta^\Psi \equiv E(\phi_i^* | \Psi = 1)$ .



Table 5: Information and knowledge about malaria

Variables	Treatment	Control	$E(Y T = 1, X) - E(Y T = 0, X)$		
			No Regressors	Basic Regressors	All Regressors
1. Mosquitoes mentioned among malaria vectors	0.908 (0.289)	0.854 (0.353)	0.0541** (0.0213)	0.0305* (0.016)	0.0384** (0.0158)
2. Malaria is a problem in community	0.726 (0.446)	0.670 (0.471)	0.0564 (0.0442)	0.035 (0.035)	0.0401 (0.0373)
3. Children mentioned among most affected by malaria	0.863 (0.344)	0.788 (0.409)	0.0744*** (0.0248)	0.0679*** (0.019)	0.0603*** (0.0183)
4. Pregnant women mentioned among most affected	0.367 (0.482)	0.365 (0.482)	0.002 (0.0403)	-0.0143 (0.024)	-0.00637 (0.0263)
5. In the previous 6 months, heard/saw messages about: 5a. ITNs	0.484 (0.500)	0.469 (0.499)	0.0152 (0.0421)	-0.00050 (0.038)	0.00306 (0.0359)
5b. Early seeking behavior	0.537 (0.499)	0.501 (0.500)	0.0365 (0.0420)	0.019 (0.040)	0.0184 (0.0363)
5c. Environmental management	0.450 (0.498)	0.387 (0.487)	0.0638 (0.0430)	0.029 (0.036)	0.0306 (0.0357)
Joint tests on variables (with comparable sample size):					
	1-4	p-values =	0.0103	0.0021	0.0096
	5a-5c		0.4462	0.7562	0.7463

Note: one observation per household (data available for respondents only). Columns 1 and 2 report means for treatment and control groups, with standard deviations in parentheses. Columns 3-5 report the difference between treatment and control groups, estimated using probit regression (13). The specification in column 3 does not include any controls. The specification in column 4 includes controls for: Tigre tribe, Muslim religion and subzone dummies. The specification in column 5 additionally includes all controls used in the randomization checks, i.e.: gender, age, household size, number of household members under age five, number of household members under age 18, number of rooms in dwelling, number of sleeping rooms, number of sleeping spaces; and dummy variables for whether respondent: usually lives in dwelling, was there the night before, ever attended school, is literate, is married; and dummy variables for whether main water source is: public tap, unprotected well, unprotected spring; and dummy variables for whether household owns a toilet, household owns a radio, firewood is main fuel for cooking, dwelling has no windows. In all regressions, observations are clustered at village level and robust standard errors are reported in parentheses. We also run a probit regression of treatment on sets of variables in the table plus the controls listed above and we test if these sets of coefficients are jointly We report p-values at the bottom of the table. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table 6: Ownership and use of mosquito bed nets

Variables	Treatment	Control	$E(Y T = 1, X) - E(Y T = 0, X)$		
			No Regressors	Basic Regressors	All Regressors
1. Number of nets owned by household	1.774 (1.279)	1.575 (1.207)	0.200* (0.110)	0.214** (0.0996)	0.214** (0.0837)
2. Number of ITNs owned by household	1.444 (1.206)	1.278 (1.126)	0.166* (0.0963)	0.176* (0.0926)	0.181** (0.0821)
3. Reported net use (of each household member)	0.429 (0.495)	0.380 (0.486)	0.049 (0.035)	0.034 (0.033)	0.059* (0.031)
4. Number of observed nets used the night before	1.384 (1.214)	1.164 (1.054)	0.220** (0.0990)	0.186** (0.0877)	0.170** (0.0824)
5. Number of observed nets left unused the night before	0.676 (0.993)	0.736 (1.001)	-0.0600 (0.0763)	0.0152 (0.0626)	0.00153 (0.0636)
Joint tests on variables (with comparable sample size):			1,2,4,5	p-values =	0.0659

Note: one observation per household for variables 1,2,4,5. One observation per individual for variable 3. In this table, “nets” refers to any bed nets, irrespective of their treatment status, whereas “ITNs” includes only LLINs and properly treated ITNs, following the definition presented in footnote 21. Columns 1 and 2 report means for treatment and control groups, with standard deviations in parentheses. Columns 3–5 report the difference between treatment and control groups, estimated using LS regression (12) for continuous outcomes and probit regression (13) for binary outcomes. The specification in column 3 does not include any controls. The specification in column 4 includes controls for: Tigre tribe, Muslim religion and subzone dummies. The specification in column 5 additionally includes all controls used in the randomization checks, i.e.: gender, age, household size, number of household members under age five, number of household members under age 18, number of rooms in dwelling, number of sleeping rooms, number of sleeping spaces; and dummy variables for whether respondent: usually lives in dwelling, was there the night before, ever attended school, is literate, is married; and dummy variables for whether main water source is: public tap, unprotected well, unprotected spring; and dummy variables for whether household owns a toilet, household owns a radio, firewood is main fuel for cooking, dwelling has no windows. In all regressions, observations are clustered at village level and robust standard errors are reported in parentheses. We also run a probit regression of treatment on sets of variables in the table plus the controls listed above and we test if these sets of coefficients are jointly significant. We report p-values at the bottom of the table. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Table 7: Participation in Larval Habitat Management (LHM)

Variables	Treatment	Control	$E(Y T = 1, X) - E(Y T = 0, X)$		
			No Regressors	Basic Regressors	All Regressors
Over the six month before the survey:					
1. Respondent participated in LHM	0.322 (0.468)	0.282 (0.450)	0.040 (0.044)	0.012 (0.038)	0.021 (0.037)
During the month before the survey:					
2. Days spent by household in LHM	0.632 (2.774)	0.618 (1.978)	0.013 (0.181)	0.025 (0.161)	0.094 (0.180)
3. Household members who participated in LHM	0.456 (1.007)	0.39 (0.898)	0.066 (0.077)	0.051 (0.071)	0.055 (0.065)
4. Male household members >15 years old who participated in LHM	0.167 (0.462)	0.125 (0.399)	0.042 (0.031)	0.025 (0.027)	0.030 (0.026)
5. Female household members >15 years old who participated in LHM	0.215 (0.47)	0.219 (0.483)	-0.004 (0.038)	-0.001 (0.034)	-0.008 (0.033)
6. Household members <15 years old who participated in LHM	0.075 (0.467)	0.046 (0.372)	0.029 (0.025)	0.027 (0.026)	0.033 (0.025)
Joint tests on variables (with comparable sample size):	1,2,4-6	p-values =	0.3683	0.5752	0.3652

Note: one observation per household. Columns 1 and 2 report means for treatment and control groups, with standard deviations in parentheses. Columns 3-5 report the difference between treatment and control groups, estimated using LS regression (12) for continuous outcomes and probit regression (13) for binary outcomes. The specification in column 3 does not include any controls. The specification in column 4 includes controls for: Tigre tribe, Muslim religion and subzone dummies. The specification in column 5 additionally includes all controls used in the randomization checks, i.e.: gender, age, household size, number of household members under age five, number of household members under age 18, number of rooms in dwelling, number of sleeping rooms, number of sleeping spaces; and dummy variables for whether respondent: usually lives in dwelling, was there the night before, ever attended school, is literate, is married; and dummy variables for whether main water source is: public tap, unprotected well, unprotected spring; and dummy variables for whether household owns a toilet, household owns a radio, firewood is main fuel for cooking, dwelling has no windows. In all regressions, observations are clustered at village level and robust standard errors are reported in parentheses. We also run a probit regression of treatment on sets of variables in the table plus the controls listed above and we test if these sets of coefficients are jointly significant. We report p-values at the bottom of the table. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Table 8: Behaviors conducive to malaria eradication, other than LHM

Variables	Treatment	Control	$E(Y T = 1, X) - E(Y T = 0, X)$		
			No Regressors	Basic Regressors	All Regressors
1. Household keeps livestock > 100m from home	0.807 (0.395)	0.776 (0.417)	0.031 (0.032)	0.068** (0.031)	0.067** (0.030)
2. Household covers stored water	0.942 (0.234)	0.953 (0.212)	-0.011 (0.020)	-0.027 (0.018)	-0.019 (0.017)
3. Respondent does anything to prevent mosquito bites	0.834 (0.372)	0.804 (0.397)	0.030 (0.031)	-0.006 (0.025)	0.007 (0.025)
4. Respondent mentions using net	0.680 (0.467)	0.649 (0.478)	0.029 (0.039)	0.011 (0.029)	0.027 (0.030)
5. Respondent mentions burning coils	0.225 (0.418)	0.211 (0.409)	0.015 (0.035)	0.003 (0.022)	-0.002 (0.023)
6. Respondent mentions using spray	0.025 (0.156)	0.021 (0.143)	0.004 (0.009)	0.010 (0.008)	0.010 (0.007)
7. Respondent mentions burning animal dung	0.058 (0.234)	0.046 (0.209)	0.012 (0.014)	0.005 (0.012)	0.004 (0.011)
8. Respondent mentions burning herbs	0.048 (0.215)	0.054 (0.226)	-0.006 (0.018)	-0.017 (0.014)	-0.013 (0.012)
9. Respondent mentions draining stagnant water	0.106 (0.309)	0.120 (0.325)	-0.014 (0.021)	-0.022 (0.018)	-0.016 (0.016)
Joint tests on variables (with comparable sample size):			0.8851	0.5764	0.4199
			p-values =		

Note: one observation per household. Columns 1 and 2 report means for treatment and control groups, with standard deviations in parentheses. Columns 3–5 report the difference between treatment and control groups, estimated using probit regression (13). The specification in column 3 does not include any controls. The specification in column 4 includes controls for: Tigre tribe, Muslim religion and subzone dummies. The specification in column 5 additionally includes all controls used in the randomization checks, i.e.: gender, age, household size, number of household members under age five, number of household members under age 18, number of rooms in dwelling, number of sleeping rooms, number of sleeping spaces; and dummy variables for whether respondent: usually lives in dwelling, was there the night before, ever attended school, is literate, is married; and dummy variables for whether main water source is: public tap, unprotected well, unprotected spring; and dummy variables for whether household owns a toilet, household owns a radio, firewood is main fuel for cooking, dwelling has no windows. In all regressions, observations are clustered at village level and robust standard errors are reported in parentheses. We also run a probit regression of treatment on sets of variables in the table plus the controls listed above and we test if these sets of coefficients are jointly significant. We report p-values at the bottom of the table. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Table 9: Intra-household allocation of bed nets

Subsamples	Y = 1 (Net Use)			
	Treatment	Control	No Regressors	Basic Regressors
Children under 5	0.5292 (0.4995)	0.4970 (0.5004)	0.0323 (0.0471)	0.0174 (0.0394)
Youth aged 5–20	0.4107 (0.4921)	0.3623 (0.4808)	0.0484 (0.0393)	0.0327 (0.0382)
Adult male workers	0.3520 (0.4781)	0.2697 (0.4443)	0.0823** (0.0407)	0.0841** (0.0420)
Adult female workers	0.5000 (0.5013)	0.4026 (0.4915)	0.0974* (0.0544)	0.0695 (0.0568)
Adult male unemployed	0.3000 (0.4594)	0.2409 (0.4286)	0.0591 (0.0556)	0.0570 (0.0564)
Adult female unemployed	0.4714 (0.4996)	0.4408 (0.4969)	0.0306 (0.0452)	0.0132 (0.0405)
				0.0246 (0.0403)
				0.0625* (0.0332)
				0.1134*** (0.0425)
				0.1313** (0.0611)
				0.0793 (0.0549)
				0.0269 (0.0444)

Note: The outcome variable  $Y$  is an indicator variable =1 if individual reportedly slept under a bed net the night before the survey, and =0 otherwise. For each subsample, columns 1 and 2 report average bed net use in treatment and control villages, with standard deviations in parentheses. Columns 3–5 report the difference between treatment and control groups, estimated using probit regression (13). The specification in column 3 does not include any controls. The specification in column 4 includes controls for: Tigre tribe, Muslim religion and subzone dummies. The specification in column 5 additionally includes all controls used in the randomization checks, i.e.: gender, age, household size, number of household members under age 5, number of household members under age 18, number of rooms in dwelling, number of sleeping rooms, number of sleeping spaces; and dummy variables for whether respondent: usually lives in dwelling, was there the night before, ever attended school, is literate, is married; and dummy variables for whether household owns a toilet, household owns a radio, firewood is main fuel used for cooking, dwelling has no windows. In all regressions, observations are clustered at village level and robust standard errors are reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .